Employment Exposure: Employment and Wage Effects in urban Malawi

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Abstract

Labor earnings are critical to exiting poverty. Understanding the returns to the determinants of wage growth is thus important. This paper examines the role of one driver of wage growth, acquired work experience. We exploit an experiment that randomized probabilistic job offers to estimate the employment and wage effects of short term jobs among young men in an urban low income setting. The results suggest large returns even among relatively well-educated yet still under-employed individuals. Individuals are more likely to access subsequent employment in the position for which they obtained work experience, suggesting a perceived return to the experience by other firms. Returns are largest among those scoring poorly on a literacy and numeracy test and those with existing sector specific evidence.

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1. Introduction

Extensive research shows that positive labor-related events are critical to exiting poverty, while job losses or the dearth of job opportunities prevents such mobility (Inchauste, 2012; Baluch, 2011; Fields et al., 2003). Furthermore, the WDR (2013) documents that being employed is insufficient to exit poverty, rather, increased labor earnings are necessary. To better inform pro-poverty reduction policies, it is important to understand the key determinants of wage growth specifically among the youth. One driver of wage growth is acquired work experience. Both firm-specific and general work experience have been documented to be important sources of wage growth.[1] Youth, who have acquired the least experience are also most at risk of unemployment and future wage growth. One potential reason for this is inefficient labor markets for entry-level workers (as documented in Pallais (2013) in the context of oDesk).

This paper contributes to this literature in an urban low income developing country context. It examines the effect of short -term work experience with a private employer on employment and wages in urban Malawi. The sample of relatively inexperienced youth provides a novel opportunity to address this question. Empirically this is challenging as work experience is correlated with other factors that affect employment or wages that are not observable. For example, individuals who acquire work experience may exhibit better non-cognitive skills not observable in the data.[2] To overcome this identification challenge we exploit an unusual source of random variation in short term employment from another experimental study, discussed in detail in Godlonton (2014). The experimental study randomly allocated a probabilistic chance of short term employment in a real job during a real recruitment process. The randomly determined employment options provide a

[1] There is debate regarding how large the effects of job-tenure are but there is consensus on the sign of the effect (Altonji and Shakotko, 1987; Topel, 1991; Altonji and Williams, 1997; and Buchinsky et al. 2010). There is also a debate regarding the impact of in-school labor market experience on wages in the United States. Most studies find a sizeable labor market payoff to this type of experience (Meyer and Wise, 1982; Coleman, 1984; Ruhm 1995 and 1997; and Light, 1999 and 2001).

[2] Several papers have shown that non-cognitive influence labor market outcomes (Bowles, Gintis, and Osborne 2001; Jacob 2002; Heckman, Stixrud and Urzua, 2006).

suitable instrument for acquired short term work experience. By instrumenting for an individual's work experience using his randomly-assigned chance of gaining experience from the short term job, we estimate the effect of short term work experience on employment and wages.

This approach overcomes an additional common problem inherent in measuring the returns to experience in developing countries – the dearth of detailed work experience data that would enable one to measure experience accurately rather than relying on an experience proxy (such as age-years of schooling - 6). We utilize employment history data for the eight month period following the experiment, and importantly measure actual rather than "potential experience". While potential experience is considered a poor proxy in general, the prevalence of interrupted or delayed schooling and periods of unemployment renders potential experience an even poorer proxy for actual experience in developing countries (Lockheed, Verspoor, et al. 1991; Lam, Ardington and Leibbrandt 2011; and Pugatch, 2013).

We find a positive impact of acquired short term work experience on employment, albeit imprecisely estimated. The work experience opportunity increases average employment by approximately 11 percentage points during the post-intervention period. We also find a sizeable (and statistically significant) wage return to work experience. Wages increase by about 67 percent conditional on being employed.

These are large wage returns, and using ancillary data collected we attempt to explore the mechanisms leading to such large returns. First, we examine occupational changes and observe effects consistent with a shift away from agriculture and related occupations towards clerical and related occupations. However these shifts are not statistically significant. We do find strong evidence that individuals are more likely to have worked as a research assistant, the job for which they acquired work experience in the post intervention period. Second, individuals are more likely to be employed in less permanent jobs (proxied by pay period unit). Third, we observe important heterogeneity in the observed employment and wage effects. Individuals of lower ability (as

assessed by a numeracy and literacy test) benefit the most from the work experience. For this subgroup, the effect of experience on the probability of employment is statistically significant. This is consistent with potential inefficiencies in the low skilled sector of the urban labor market induced by employers hiring based primarily on test scores, be it the national secondary school examination results (MCSE) or other recruitment tests. Lastly, we also note that the short term work experience obtained here appears complementary to existing sector and employer type experience as those with such experience incur larger returns.

Other potential mechanisms through which experience leads to wage increases are explored. The data do not support the hypotheses that expanded social networks, signaling of ability from letters of reference, or increased reservation wages drive the observed wage increases.

These results add to the policy debate about active labor market programs, which are designed to improve employment outcomes by providing participants with work experience. The empirical evidence on such programs provides mixed results. In systematic reviews of the literature, the key take away is that the impact of job-training programs are modest at best (Heckman, Lalonde, Smith, 1999; Kluve, 2006). However, more recently, Pallais (2013) finds large employment effects in the context of short term experience through oDesk, Furthermore, just like the returns to education, the impacts of such programs might be larger in low income countries. Betcherman, Olivas and Dar (2004) review the literature about impact evaluations of job training programs and find only 19 studies (none of which are in Africa) conducted in developing countries. In both this review and in another, by Nopo and Saavedra (2003) of the non-experimental literature in Latin America, the estimated impacts of job training programs appear to be larger in developing than developed countries.

The paper is organized as follows. Section 2 presents the experimental variation and data used. Section 3 presents the empirical strategy, and the main results. Section 4 examines and discusses potential mechanisms, while Section 5 concludes.

2 Experiment and data

2.1 Experimental variation

The experiment was a collaborative effort between a local independent recruiter and the research team. The sample of respondents is drawn from a recruitment process hiring male interviewers, during which trainees also participated in an experiment that offered randomly determined probabilistic jobs. The recruiter posted advertisements to recruit individuals for short term interviewer positions. Interested applicants who met the eligibility criteria (male, aged 18 and older, completed secondary schooling, and arrived punctually for initially screening assessment test) were required to write an initial assessment test and encouraged to submit their resume. The top-performing applicants, 278 individuals, were offered an opportunity to participate in the extended training and recruitment process. Figure 1 outlines the timeline of the data used in this paper.

Consenting individuals (N=268) participating in the real training and recruitment process were offered a probabilistic chance of an alternative employment opportunity. Individuals were assigned a 0-, 1-, 5-, 50-, 75- or 100-percent chance of alternative employment in the event that they failed to secure employment through the recruiter's competitive hiring process. The alternative employment opportunity offered the same duration and wage as the standard employment offer from the recruiter.[3] The recruiter's job and the alternative jobs were of equal duration and paid the same wage. Thus, those who became employed through the project acquired the same amount of work experience at the same pay whether they ultimately worked for the recruiter or in the alternative job. Estimation of the effect of the probabilistic jobs must account for the fact that they increased the likelihood of both being selected for the recruiter's job and being eligible for the alternative job (see Godlonton, 2014 for details).

[3] Individuals were still able to earn a job through the recruitment process by performing well during the job training, and those who secured both jobs were required to take the recruiter's job or turn down both job offers.

Once the recruitment process was completed, the probabilistic chances of employment were realized. For individuals assigned a 1-, 5-, 50- or 75 percent chance of an alternative job; random draws were conducted.[4] For this paper we use the treatment assignment (i.e. the probability of an alternative job) to instrument for acquired short term work experience. This unusual determination of employment allows a novel opportunity to measure the causal effect of short term work experience on future labor market outcomes in an urban low-income context.

The work experience opportunity is short term. The job provided individuals with five days of paid work experience. The recruiter's job was for employment as an interviewer. The alternative jobs were different research assistant tasks, including archival research, data entry, and translation and transcription of qualitative interviews. Many of these tasks may embody some real acquisition of new and transferable skills for the participants. Upon completion of the job, participants received a generic letter of reference.

2.2 Data

Data come from a baseline survey collected prior to the start of the recruitment process, administrative records about treatment assignment and employment realizations for both probabilistic alternative jobs and hiring by the recruiter, and a follow-up survey that was conducted nine months after the completion of the work opportunities presented by the experiment.

Baseline data: Prior to the start of the recruitment process, respondents completed numeracy and literacy tests and submitted their resumes. These tests are used to construct an ability measure. A baseline survey complements this data, providing information on basic demographics, general education and work experience, as well as mental and physical health. The baseline survey was self-administered by respondents.

[4] For example, an individual assigned a 75-percent chance of an alternative job drew a token from a bag that contained 75 red tokens and 25 green tokens. If the individual drew a red token then he was offered the alternative job; if he drew a green token, he was not. Similar draws were conducted by each individual, with token distributions adjusted for his randomly-assigned probabilistic treatment groups. Individuals assigned a 0-percent chance knew with certainty they were not eligible for alternative jobs and those assigned a 100-percent chance knew they were guaranteed alternative jobs, so no draws were conducted in those cases.

Probabilistic alternative job offers: The analysis uses both the assignment to treatment records, as well as the realization of the probabilistic draws (i.e. whether or not each participant was actually offered a job). Assignment to an employment probability was stratified by baseline ability quintile and prior experience with the recruiter. There are no systematic differences in covariates between the different treatment groups (Godlonton, 2014).

Follow-up survey data: A follow-up survey was conducted nine months after the implementation of the experiment. While the reference period for the survey is the nine month period following the completion of the work experience opportunity, some participants erroneously report work tied to the experiment a month after it was completed. To deal with this survey recall error, we exclude the first month of recall data and rely only on the eight month period beginning one month after the completion of the work experience opportunities.[5] The follow-up survey was conducted by phone and included an extensive module on job search, labor market perceptions (current and future likelihood of finding employment), current employment and employment experiences over the last eight months, current and past wages.

Table 1 shows that attrition is not statistically significantly associated with treatment status. A total of 84.7 percent of the sample was successfully interviewed at follow-up. The attrition rate is lowest among participants who had received the 75-percent job guarantee (7.1 percent) and highest among those receiving a 0-percent chance of an alternative job (18.9 percent). The difference in attrition between these two groups, although large, is not statistically significant (p=0.168). Moreover, the probability of receiving an alternative job does not predict the probability of being interviewed at follow-up (coeff. = 0.049, p-value = 0.433).

In Table 2 we show there is not differential attrition for many other baseline characteristics including age, education, ability and previous work experience (Column 5). Respondents of the Ngoni tribe and those that had worked in the six months prior to baseline are slightly less likely to

[5] All results are qualitatively similar when including the first month following the employment opportunity.

attrit (significant at the 5 percent level and 10 percent level respectively). However, these differences are not large in magnitude. There is no systematic differential attrition by treatment status (i.e. the probability of the alternative job) that is correlated with baseline characteristics.[6]

The final analytical sample includes the 227 respondents found at follow-up. The average respondent in this sample is approximately 26 years old and 17.2 percent are married. Approximately 16.7 percent of the sample have at least one child, and of those that do have at least one child they have an average of 1.8 children. Respondents are relatively well educated for Malawi with an average of 13 years of education, but this is driven by the eligibility criteria of the recruiter which required candidates to have completed secondary school education. Despite being relatively well-educated for Malawi all men in the sample were actively seeking work at the time of the baseline survey. They report earnings of approximately $210 per month spanning the three month period prior to the experiment (Table 2, Column 2).

3 Empirical strategy

If experience was randomly assigned across individuals, then we could estimate the average treatment effect of experience on employment and wages using ordinary least squares (OLS). In that case, one would estimate the following regression equation:

$$y_i = \alpha + \beta_1 JO_i + X_i'\delta + \varepsilon_i \qquad (1)$$

where y_i = employment (or wages) for individual i, JO_i is a dummy indicator for whether or not the individual was offered a job. X_i represents a set of covariates.

However, in this setting work experience was not itself randomly assigned. Instead, individuals were randomly assigned different probabilities of obtaining work experience. These probabilistic job guarantees affected their likelihood of obtaining experience from one of two

[6] To test this, we regress an indicator for being in the follow-up sample on the probability of being assigned an alternative job, the baseline characteristic of interest, and that probability interacted with the baseline characteristic (Appendix Table 1).

different types of jobs – the recruiter's job and the alternative job. We therefore implement an instrumental variables approach. The system of equations then estimated is:

$$Y_i = \alpha_0 + \beta_1 AnyJO_i + X_i'\delta + \varepsilon_i \qquad (3)$$

$$AnyJO_i = \pi_0 + \pi_1 P1_i + \pi_1 P5_i + \pi_1 P50_i + \pi_1 P75_i + \pi_1 P100_i + X_i'\varphi + \varepsilon_i \qquad (4)$$

where $AnyJO_i$ measures whether individual *i* was offered a short term job; $P1_i, P5_i, P50_i, P75_i, P100_i$ are binary indicators for the different treatment arms; X_i is a set of individual-specific covariates. The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months (at baseline) he has worked. We include stratification cell fixed effects to account for the stratification of treatment assignment by ability and prior work experience with the recruiter. The key coefficient of interest is β_1. Conditional on instrument validity, β_1 captures the local average treatment effect (LATE) of the short term job on labor market outcomes – employment and wages. We allow for possible heteroskedasticity in the error terms by using heteroskedastic-robust standard errors.

Y_i measures labor market outcomes of interest. We examine the extensive margin using an indicator for ever being employed and the share of months employed in the subsequent eight month period. To examine effects at the intensive margin we focus on the number of days worked and the average daily wage earned by individual *i* across that the eight month period. We use day as the relevant time unit as this is most appropriate in the local context. Institutionally, Malawian labor policies pertain to daily employment; for example, the minimum wage law is with respect to daily wages, not hourly wages.[7]

[7] Daily or even more highly aggregated wages are also salient to respondents. The follow-up survey allowed individuals to choose the time unit for reporting their wages, with, 75.8 percent of respondents reporting monthly wages and 18.5 percent reporting daily wages.

For the randomized outside option probabilities to serve as a valid instrument for work experience, it needs to satisfy two conditions: the instrument must be correlated with the endogenous variable; and the probabilistic job offers must not affect later labor market outcomes except through the acquired work experience.

The first condition implies that the assigned probability of alternative employment should predict whether or not the job-seeker acquired any job (recruiter or alternative job) through this intervention. Estimating the first stage relationship shows that the instrument is, indeed, relevant (Table 3). The probabilistic outside options strongly predict the probability participants received any job (recruiter or alternative). This expected result derives mechanically from the assignment of alternative jobs, as well as through a behavioral response by participants to the job guarantees. As shown in Godlonton (2014) the probability of being hired by the recruiter was higher among those who received the 75- or 100- percent chance of an alternative job, likely because the improved outside option lowered stress and increased performance during the recruiting process. Both mechanisms work in favor of a higher probabilistic job guarantee causing a higher chance of subsequent employment. Table 3 Column 1 confirms this pattern. A total of 16.3 percent of individuals assigned a zero chance of an alternative job got a job. Individuals assigned a 1- or 5-percent chance of an alternative job are not more likely than those who were assigned a 0-percent chance to get any job. The coefficients are positive as predicted, though the standard errors are large. Individuals assigned a 50-, 75- and 100- percent chance of an alternative job are respectively 40.2, 56.8 and 83.7 percentage points more likely to get any job than those with no chance of the alternative job. The first stage F-statistic is 101.11, far above the rule of thumb threshold for weak instrument concerns. These results are robust to the inclusion of stratification cell fixed effects (column 2) and additional covariates (column 3).

The exogeneity condition for the IV strategy requires that, conditional on baseline characteristics, the probabilistic job offers do not affect later employment outcomes independently

of acquiring a job through the experiment (recruiter or alternative). Monotonicity would be violated if higher probabilistic job offers had reduced the likelihood of acquiring the recruiter's job. However, as shown in Godlonton (2014) this is not the case. In fact, individuals assigned a 75- or 100 –percent chance of an alternative job were about twice as likely to be hired by the recruiter as those who were not eligible at all for alternative jobs.

A second concern is that the probabilistic job offers may have affected individuals' perceptions about their own ability to find employment. Godlonton (2014) finds no impact of the probabilistic job offers on the perception of ones' own likelihood of employment.

A third concern is that the probabilistic job offers affected skill acquisition during training, and that skill was subsequently rewarded by the labor market. The finding in Godlonton (2014) that individuals perform differentially on recruiter administered training tests during the recruitment process may initially heighten that concern. However, it is unlikely that there were general benefits to this training. The training conducted by the recruiter and evaluated in the performance tests was tailored to the specific needs of that particular recruiter's temporary job, interviewer positions for a health survey. Participants worked systematically through the questionnaire the recruiter planned to administer, in order to understand the terminology of and instructions for filling in each item. Skills related to this particular questionnaire are highly firm and project-specific and are unlikely to be marketable to the labor market. Moreover, for the training to have an impact in the labor market the differential performance of the participants needs to be observable to future employers prior to employment. Individuals did not receive their grades on these assessment tests and letters of reference only described the nature of the job but not the employee's specific performance. As such, the only way for the differential performance during training to affect subsequent employment and earnings in the outside labor market after the intervention is for outside employers to value the specific content of the training conducted by the

recruiter during the experiment. This is unlikely.[8] Generally, in this context when individuals apply for a new interviewer position even within the same firm they still are required to undergo training. In other words, experienced and novice interviewers undergo the same training for each survey they work on.

4 Results

4.1 Employment and wage effects

Table 4 presents the impact of the short term work experience on employment, job search and the concurrent number of jobs held. Outcomes are aggregated by individual across the eight month post-intervention time period. The key employment variable used is the probability of employment during this timeframe.[9] Similarly, the job search variable is defined as the average probability an individual actively sought work (whether or not they were employed).[10] The concurrent number of jobs held is constructed as the average number of concurrent jobs held during the last eight months.

Short term work experience increases the probability of subsequent employment by 8.4 to 11 percentage points. The estimated coefficients increase in magnitude and precision when we include stratification cell fixed effects (column 2) and covariates (column 3). The estimated effect is large, approximately a 26 percent increase in the probability of being employed (albeit statistically insignificant). Figure 2 plots the estimated employment impacts of the job separately for each of the eight months following the intervention. Although the one-month estimates are imprecise, the effects are positive in each of the eight months. We also find an increase in the probability of

[8] We restrict the analysis by excluding those assigned the 100-percent treatment group; and those assigned the 0-percent treatment group. These sub-groups show that the results are slightly smaller and in some cases lose statistical significance which is not surprising as the sample sizes are small. These estimates also show that the results are not eliminated by dropping either of these groups which suggests that the results are not driven by differential learning (results not shown).

[9] This is constructed by calculating the fraction of months that the individual is employed over the eight months following the intervention.

[10] This is constructed as the fraction of months an individual actively sought work in the post-intervention period.

searching for a job (columns 3 and 4) and a reduction in the number of concurrent jobs held (columns 5 and 6), but these coefficients are not statistically significant.

Underemployment in Malawi is high, as such, there is considerable scope to increase labor supply along the intensive margin. Data from a nationally representative household survey shows that urban men who have completed secondary school, the relevant comparison group for the experimental sample, work only 23.4 hours per week conditional on being employed. In this context, individuals are more likely to be able to adjust their labor supply at the daily rather than hourly margin, and they are paid per day rather than per hour. Therefore, our preferred specifications pertain to the number of days worked and daily wages as presented in Table 5 Columns 1 through 9. Individuals induced into work experience from the experimental job probabilities work on average one additional day per week (Column 3), and earn $3.94 more per day (Column 6). This is large implying an 80 percent increase in daily wages. The logged wage results also exhibit a large wage return (but as expected smaller in magnitude) of 67 percent.[11]

Month-by-month estimates are plotted in Figure 3. In all months, the effect on daily wages is positive; ranging from approximately one to six dollars. Due to the imprecision of the estimates, despite the large range of effect sizes across months the individual monthly estimates are not statistically different from one another. The estimated wage impacts are large. Part of the increase in wages is attributable to the gains in employment as shown in Table 4 and increased number of days worked.

These effects are much larger than those obtained from non-experimental Mincerian estimates in Malawi and other similar settings.[12] However, they are comparable to a recent experimental study (Pallais, 2013) in the context of low-skill work online (oDesk). There are

[11] It is worth noting that the large point estimates are not driven by outliers. Appendix Figure 1 documents the wage distributions for those who did and did not receive a job and shows that the wage distribution among those who received a job is shifted to the right.

[12] The estimated wage returns in this paper are equivalent to approximately 10 years experience in the Malawi non-experimental estimates obtained by Chirwa and Matita (2009).

several reasons why the non-experimental estimates may be substantially smaller. Non-experimental estimates typically use an inferior (but readily used and available) measure of work experience. Potential experience overstates the amount of accumulated experience (considerably) in this context. Second, the type of experience studied by the experiment may be of higher quality than the average experience obtained in the labor market. Experience provided through the experiment was short term, it was with a private, international employer. It is unlikely that five days of work in the civil service will yield impacts similar to that observed here. Also, the non-experimental estimates represent average returns to experience for a population that is less educated than the highly-skilled men included in the experiment. While the experimental subjects still experience frequent periods of unemployment, they may experience substantively different returns than a less educated counterpart.

4.2 Heterogeneity of impacts

We turn next to examine the heterogeneity of the estimated effects. We explore heterogeneous returns by ability and several dimensions of prior experience. To do so, we interact an indicator variable for having received an alternative job (JO_i) with the baseline characteristic of interest ($Base_i*JO_i$), using the set of treatment dummies as instruments for work experience. In this specification we instrument the endogenous regressors with the probability of an alternative job and this probability interacted with the baseline characteristic.

Table 6 Column 1 examines the heterogeneity of impacts by ability as measured by the composite of test scores from a numeracy and literacy test administered to the respondents at baseline. A composite measure of ability (numeracy and literacy test scores) is used.[13] Estimated impacts are larger for individuals at the lower end of the ability distribution. As an example, consider an individual at the 25th percentile and the 75th percentile of the ability distribution. Individuals at the 25th percentile were 25 percentage points more likely to be employed if they

[13] The results are similar when using the numeracy and literacy scores separately.

were induced to receive job experience through the experiment, and they earn approximately $11.01 more per day. On the other hand, individuals at the 75th percentile were 1.5 percentage points less likely to be employed, though they earn approximately $2.20 more per day. This shows significantly larger wage returns among those scoring poorly on a written test.

Table 6 Columns 2 through 4 examine the extent to which effects vary by different prior experience. Average estimated wage returns may be so large because it is the first job held by respondents. Roughly 15 percent of the sample had no previous work experience. Perhaps surprisingly, the effects of work experience on subsequent employment do not differ by pre-experimental work experience. Interestingly, the wage returns are magnified for those with either international employer or existing research experience, suggesting that sector specific experience is complementary in this context.

4.3 Why such large estimated returns?

To further understand the large estimated wage returns we explore a number of potential pathways for the subsequent labor outcomes. First, we examine whether during the post-intervention time period individuals switched into higher paying occupations. Second, we examine elements of contracting, specifically the time unit in which wages is reported as a proxy for the permanence of the job. Third, we examine whether social network referrals and reference letters were differentially used. Changes to social networks and access to a reference letter may also open up new higher paying job opportunities without necessitating an occupational shift. Fourth, we consider whether reservation wages were altered which may have altered the likelihood of accepting low paying wage offers.

Occupation shifts

To examine whether the short term work experience led to occupation shifts we use the retrospective calendar job histories, and categorize jobs using the standard two-digit ILO occupation classification codes (ISCO-08 classification system). We examine two constructed

measures of occupation-specific employment: a binary indicator for whether each individual worked in a given occupation; and the total number of months the respondent worked in each occupation.

In Table 7, each row reports the effect of work experience on employment in a separate occupation from an IV regression. Panel A corresponds to the binary ever-worked outcome; and Panel B corresponds to the number of months in the occupation. Limited statistical power inhibits the ability to make strong claims for the observed occupational shifts. However, the pattern of results suggests that work experience increases employment in both administrative and managerial; and clerical and related work and reduces employment in the agriculture and related occupations.[14]

To complement the standard classification system we also consider the impact on subsequent employment as a research assistant in the post intervention period. Individuals acquiring work experience were 15.4 percentage points more likely to have worked as a research assistant, the specific occupation in which they acquired experience, post-intervention (Table 7 Panel A Column 8). Similarly, they were likely to have held such a position for almost one additional month (Table 7 Panel B Column 8).

Job Permanence

Jobs vary in their duration, and short term positions are common in Malawi. To proxy for job permanence we utilize information from the unit in which individuals reported their current pay unit. Individuals self-reported the unit of payment for their current (primary) job at the daily, weekly, fortnightly or monthly level. We infer that lower-frequency reporting levels correspond to longer duration contracts, and construct a frequency of payment variable equal to one if the

[14] The same pattern is observed for the modal occupation held (results not shown).

individual reports daily remuneration, two if weekly, three if fortnightly and four if monthly remuneration is reported.

Table 8 Column 1 reports the effects of work experience on this proxy for job permanence. The negative coefficient suggests individuals induced to receive work experience through the experiment work in less permanent positions. This is consistent with higher wages in the local context as wages for short term positions as research assistants or consultants on projects for international NGOs or donor agencies are often much higher than wages paid for the permanent jobs offered by local employers or government agencies. These results also suggest that informal labor markets are more limited by information inefficiencies than formal labor markets.

Reference Letters

Another potential mechanism for why such a short work experience may generate such large wage returns is potential information constraints on the employer side. To test for this we examine the use of reference letters. Employers may not infer any inherent value of the work experience on worker productivity, but merely interpret it as a signal of ability (Spence, 1973). Upon completion of the work experience all participants received a standard letter of reference, which described the job in general terms but did not provide information about individual-specific performance. Given that these letters came from an international employer, however, employers may value the letter as a signal of underlying ability, rather than certification of skills acquired through experience.

Table 8 column 2 shows that those who received work experience as a result of the experimental treatment were actually 8.6 percentage points *less* likely to use a reference letter when applying to a job.[15] Consistent with this result, the average number of times a reference letter

[15] Individuals who received work in the alternative job and those who worked for the recruiter received reference letters as such it is possible that individuals who did not receive the randomly determined job used a reference letter. However, the large difference is not too surprising as a low fraction of those who received no alternative job offer worked for the recruiter, and therefore did not receive any reference letter that could be used for this purpose.

was used to support a job application was lower for those receiving experience, although neither result is statistically significant. Given these results, employers could not respond to any potential signal value of the reference letters, and these letters do not seem to drive the subsequent labor market outcomes.

Social networks

Social networks have been touted as an important mechanism through which individuals acquire employment opportunities.[16] For the job-seeker, social connections can reduce search costs and lead to better quality matches (Calvo-Armengol, 2004; Mortensen and Vishwanath, 1994; Galeotti and Merlino, 2009). This could in turn lead to higher paying wage opportunities. Simply participating in jobs provided by this experiment may have facilitated new social connections between participants.

Unlike the experiments undertaken by Beaman and Magruder (2012) and Beaman et al. (2013) that are specifically set up to test various aspects regarding the role of social connections in job referrals, this experiment was not designed to induce variation in social connections. We do measure the prevalence of social interactions that may have facilitated employment, such as whether individuals heard about job opportunities through individuals they met during the job opportunity, and whether the jobs they held during the eight month period following this job opportunity were a direct result of a referral.

Table 8 column 4 shows that individuals who received work experience as a result of the experiment are 22.5 percentage points more likely to have heard about a work opportunity through someone they met during this intervention. On the other hand, individuals are not more likely to secure employment through one of the new connections (Table 8 Column 5).

[16] See for example Beaman (2010) and Granovetter (1973).

In sum, while the broadened network does suggest a modest impact on information about job opportunities, this information does not seem to translate into employment and is unlikely responsible for the large wage returns.

Wage expectations

A further potential mechanism through which work experience may have increased subsequent labor market outcomes is through altered wage expectations and reservation wages, with implications for job search strategies, duration of unemployment, and match quality. The wages paid during this experiment may have been higher than reservation wages at baseline. If individuals updated their expectations by increasing their reservation wage, then the estimated impact on the employment effect might be muted, as individuals may be searching longer and differently for better paying jobs.

To examine this mechanism we consider self-reported reservation wages, the results of which are presented in Table 8 Column 6. The impact of receiving a job on the monthly reservation wage is $123.91, but it not statistically significant at conventional levels. More generally, the reported reservation wages are high, approximately 1.5 times higher than the average monthly income earned at baseline. Self-reported reservation wages also high relative to wages reported in the follow up survey. There is no evidence to suggest that an increase in reservation wages is an important mechanism in this context.

Human capital accumulation

A final potential mechanism is that individuals acquired skills attributable to the work experience induced by the experiment. Individuals who secured a job either worked as an interviewer or were assigned to data entry; data transcription or translation; or archival research jobs. Results presented earlier document a suggestive change in occupational type. Individuals who received work experience are less likely to be employed in agriculture and more likely to be

employed in clerical activities. Furthermore, individuals are 18.1 percentage points more likely to report having worked as a research assistant in the post intervention period, the specific occupation in which they acquired experience. This is suggestive evidence that the work experience provided through the experiment generated occupation-specific skills that were rewarded by future employers.

While the data do not permit a direct test of the mechanism through experience increases which wages and employment, the indirect evidence suggests individuals may have acquired skills that are rewarded by the external labor market. This is further supported by the earlier heterogeneity analysis documenting which found individuals scoring poorly on written tests and those with existing sector specific experience appear to benefit the most.

5 Conclusion

This paper uses a novel experiment that generated exogenous variation in short term work experience in order to estimate the effect of such experience on employment in wages. The return to experience is large. While we find an imprecise but sizeable post-intervention employment, we document a large wage return. Individuals who received work experience earn approximately 67 percent more per day than those who did not (among those working), with results concentrated among lower-ability job candidates and those with existing sector specific experience individuals. The return to work experience persists throughout the eight month period following the intervention.

The results are large when compared to non-experimental estimates that rely on variation in potential experience. However, making direct comparisons to the non-experimental estimates is difficult given the lack of variation in the amount of experience acquired for those induced to work by the experiment. The impacts are also large relative to experimental estimates of job training

programs, which typically find modest effects at best (Heckman, Lalonde, Smith, 1999; Kluve, 2006). However, the magnitude of the results is comparable to Pallais (2013).

The results may not be generalizable to a less skilled population within Malawi, or to a country whose underlying skill distribution and labor market conditions are different from Malawi. Even within Malawi, the treatment provided in the experiment is not available through any current public or private sector job training initiatives. Because the job opportunity provided within the experiment was of uniform duration, we cannot extrapolate from these results to the return to a longer period of experience. Lastly, the general equilibrium effects of such a program are not estimated. Given the small size of this intervention, it is not possible to determine if and the extent such a program if rolled-out would have on those individuals not participating.

While these caveats cannot be dismissed, the results presented here do provide rigorous evidence about the effect of work experience on subsequent employment outcomes in an urban low income setting. The effects are substantial, suggesting that short term training or employment programs that include work experience have transformative potential, and providing justification for further research on the topic.

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Figure 1: Timeline of experiment, and data collection activities

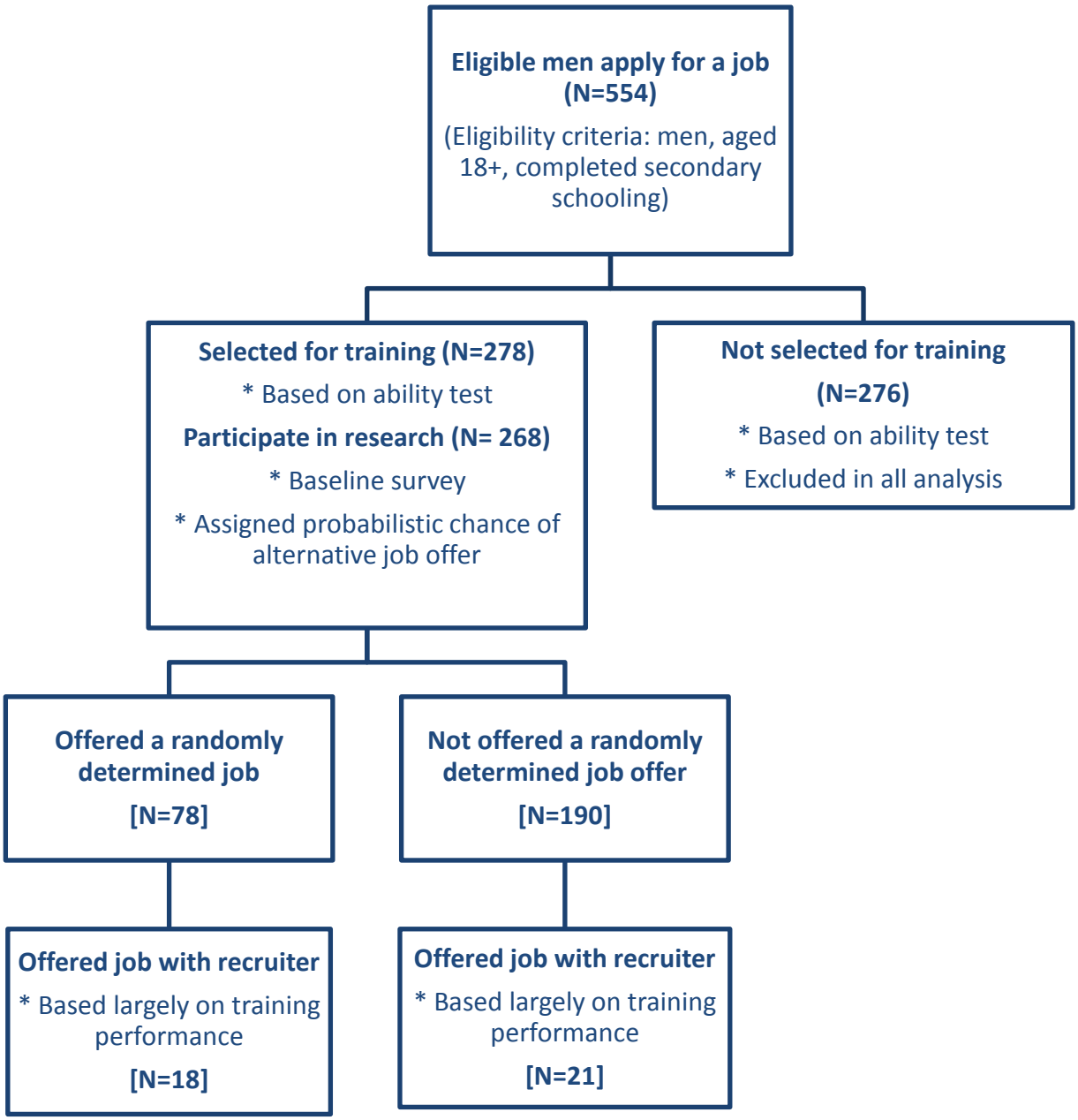


Figure 2: Estimated employment impact of job offer by month (IV estimates)

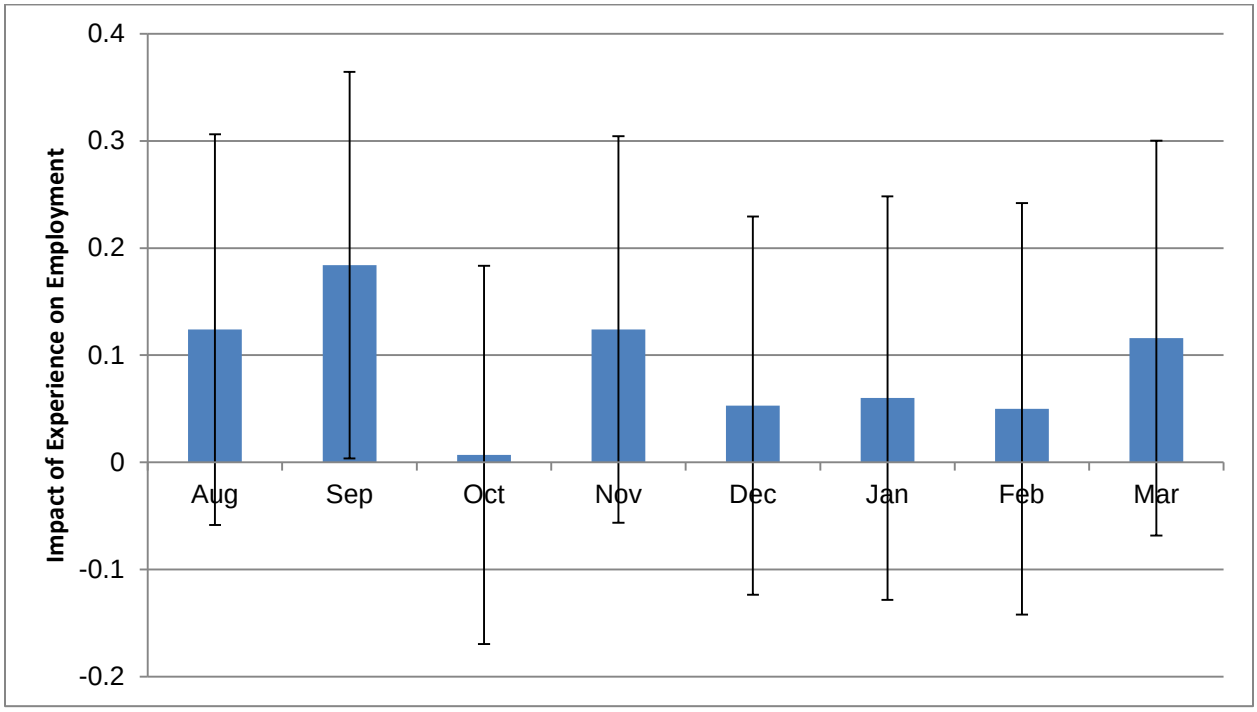
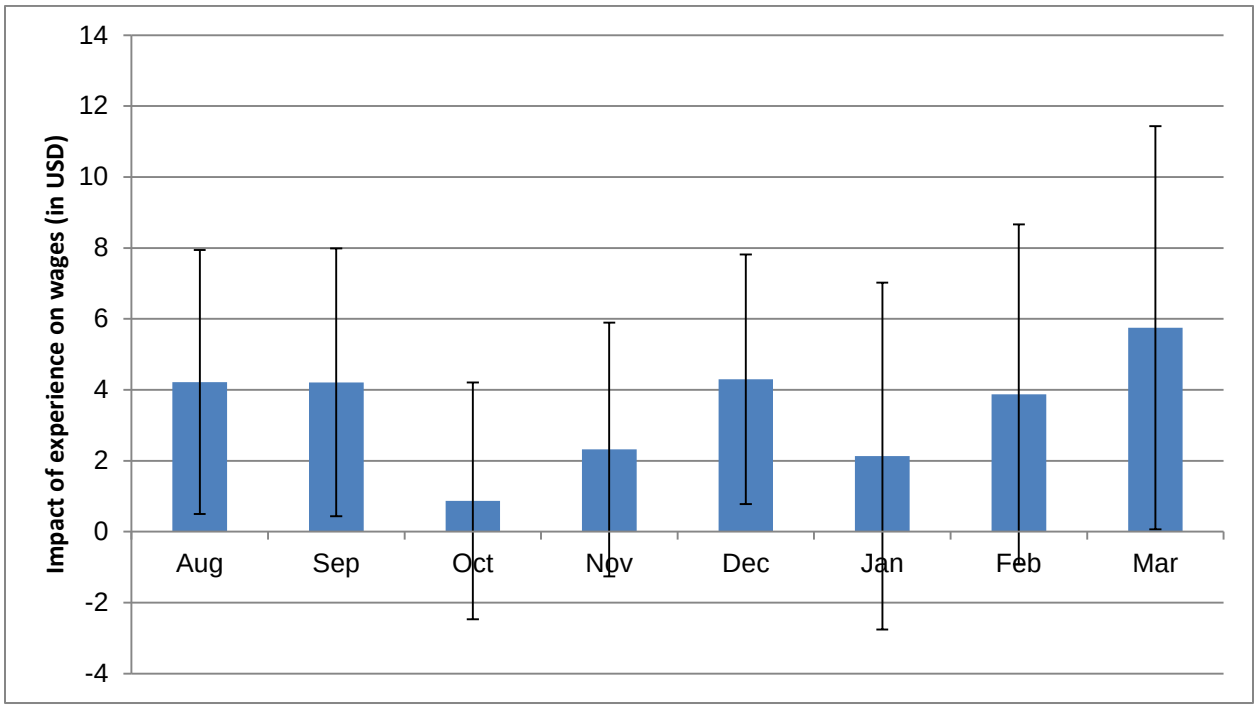


Figure 3: Estimated wage impact of job offer by month (IV estimates)



Appendix Figure 1: Distribution of wages

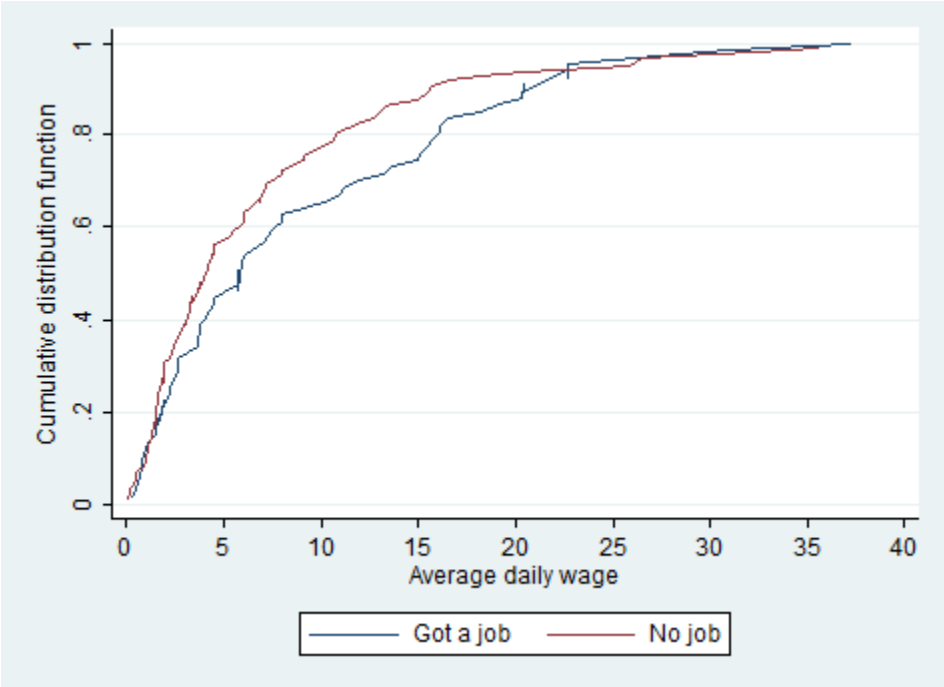


Table 1: Sample size and attrition

Treatment conditions:	N (1)	Mean (2)	SD (3)
0% Probability	53	0.811	0.395
1% Probability	56	0.857	0.353
5% Probability	52	0.827	0.382
50% Probability	54	0.852	0.359
75% Probability	28	0.929	0.262
100% Probability	25	0.840	0.374
Full sample:	**268**	**0.847**	**0.361**
p-value of F-test of joint significance:			
0% = 1% = 5% = 50% = 75% = 100%		0.827	

p-values of t-tests of pair-wise differences:

	1%	**5%**	**50%**	**75%**	**100%**
0%	0.510	0.826	0.564	0.168	0.745
1%		0.666	0.939	0.396	0.844
5%			0.724	0.233	0.882
50%				0.364	0.893
75%					0.376

Notes:

Individuals were assigned to one of the six treatment groups. If they received a 0-percent chance of an alternative (i.e. in 0% probability treatment group) then they had no chance of receiving the alternative job. If they were assigned to the 1% probability group then they had 1 percent chance of receiving an alternative job. Similarly for the 5-, 50-, 75- and 100 percent probability groups. There were twice as many assigned to the lower probability groups as compared to the lower groups due to budgetary considerations. The p-values denote the p-value associated with the F-test of whether the mean finding rate is the same in all treatment groups or in the case of the table the pair-wise t-test of differential attirion rates.

Table 2: Sample and Attrition

	Baseline N=268		Follow-Up N=228		Difference	
	Mean (1)	SD (2)	Mean (3)	SD (4)	(3) - (1) (5)	
Demographics:						
Age	25.604	4.638	25.718	4.662	-0.114	
Married	0.172	0.378	0.172	0.378	0.000	
Any child?	0.164	0.371	0.167	0.374	-0.003	
Number of children	0.299	0.784	0.313	0.811	-0.014	
Years of education	13.183	0.940	13.220	0.938	-0.037	
Income (USD, 3 months)	206.123	228.803	210.617	237.777	-4.494	
Ability score	-0.001	1.003	0.030	1.017	-0.031	
Tribe:						
Chewa	0.310	0.463	0.300	0.459	0.010	
Lomwe	0.108	0.311	0.110	0.314	-0.002	
Ngoni	0.164	0.371	0.181	0.386	-0.016	**
Tumbuka	0.190	0.393	0.189	0.393	0.001	
Other	0.201	0.402	0.198	0.400	0.003	
Education and Work:						
Ever worked?	0.869	0.338	0.863	0.344	0.006	
Work experience on cv	0.649	0.478	0.648	0.479	-0.009	
Ever worked with recruiter?	0.104	0.306	0.097	0.296	0.008	
Any work in last month	0.646	0.479	0.665	0.473	-0.020	
Any work in last 6 months	0.869	0.338	0.890	0.314	-0.020	*
Frac of 6 mths worked	2.657	2.176	2.727	2.175	-0.070	
Any job search last month	0.116	0.320	0.110	0.314	0.006	

Notes:

The baseline sample consists of 268 individuals who participated in the recruitment process and experiment discussed in Section 2 and summarized in Figure 1. The follow-up sample (227 respondents) is the main sample used in this paper. The ability score is determined prior to the experiment. It consists of a numeracy and literacy component, and has been standardized.

Table 3: First Stage results

Dependent Variable:	Job offer or recruiter's job offer		
	(1)	(2)	(3)
1% Job Guarantee	0.025	0.030	-0.005
	[0.081]	[0.078]	[0.082]
5% Job Guarantee	0.047	0.045	0.038
	[0.085]	[0.079]	[0.086]
50% Job Guarantee	0.402***	0.423***	0.443***
	[0.094]	[0.090]	[0.093]
75% Job Guarantee	0.568***	0.543***	0.568***
	[0.105]	[0.104]	[0.107]
100% Job Guarantee	0.837***	0.860***	0.864***
	[0.057]	[0.055]	[0.067]
Constant	0.163***	0.804***	0.648
	[0.057]	[0.153]	[0.481]
Observations	227	227	227
R-squared	0.327	0.382	0.431
Stratification cell FE's	No	Yes	Yes
F-stat (of instruments)	101.11	87.47	76.36
Average of dep variable		0.361	

Notes:

The main analytical sample is used. The zero percent chance of alternative employment treatment group is the omitted category in these regressions. The dependent variable "Got a job" is whether or not the individual received an alternative job offer or one of the recruiter's job offers. Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously. The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 4: Returns to Work Experience: Extensive Margin

Dependent Variable:	Pr(Employment)			Pr(Job Search)			Ave # concurrent jobs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Got a job or recruiters job	0.084	0.105	0.110	0.103	0.121	0.089	-0.028	-0.033	-0.005
offer (IV)	[0.091]	[0.090]	[0.079]	[0.083]	[0.082]	[0.074]	[0.143]	[0.165]	[0.146]
Constant	0.395***	0.395***	0.520***	-0.162	0.586***	0.754***	0.088	0.520***	0.658***
	[0.043]	[0.043]	[0.140]	[0.353]	[0.039]	[0.102]	[0.301]	[0.095]	[0.196]
Stratification cell FE's	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates?	No	No	Yes	No	No	Yes	No	No	Yes
Observations	227	227	227	227	227	227	227	227	227
Ave of dep variable (no job)		0.421			0.586			0.532	

Notes:

IV estimates are presented. Dummy indicators for treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

The probability of employment is calculated as the number of months the individual was employed over the last 8 months, divided by 8. Similarly, the probability of job search is calculated as the number of months the individual actively sought work over the last 8 months, divided by 8. Lastly, the average number of concurrent jobs is the average of the total number of jobs held each month across the 8 month period.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 5: Returns to Work Experience: Intensive Margin

	Ave days worked per week			Ave daily wage (incl. Unemployed)			Log (Ave daily wage)		
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Got a job or recruiters	0.714	0.838*	0.954**	3.909*	4.299*	3.942**	0.678*	0.693*	0.668*
job offer (IV)	[0.489]	[0.484]	[0.428]	[2.148]	[2.217]	[1.904]	[0.379]	[0.392]	[0.369]
Constant	2.172***	3.828***	-1.480	4.116***	10.675	-9.615*	1.206***	1.736***	1.089
	[0.231]	[0.900]	[1.924]	[0.863]	[7.160]	[5.687]	[0.175]	[0.584]	[1.034]
Stratification cell fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates?	No	No	Yes	No	No	Yes	No	No	Yes
Observations	227	227	227	227	227	227	165	165	165
Ave of dep variable (no job)		2.309			5.079			1.361	

Notes:

IV estimates are presented. Dummy indicators for treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

The average daily wage is calculated using the restrospective job work history. The average daily wage is calculated as the average wage on the individual's main job in the last month. Columns 1 through 3, those who are unemployed are coded as 0's. Columns 4 through 6 uses the logged wage, therefore for individuals who earned $0 across all eight months are omitted.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 6: Heterogeneity of wage and employment impacts

Panel A: Dependent variable: Probability (Employment) (8 months)

Variable:	Ability	Any experience	International employer experience	Research experience
	(1)	(2)	(3)	(4)
Got a job	0.120	0.027	0.062	0.024
	[0.074]	[0.109]	[0.084]	[0.090]
Got job X `Variable'	-0.172**	0.141	0.284	0.220
	[0.079]	[0.160]	[0.220]	[0.173]
`Variable'	0.104	0.022	-0.105	-0.027
	[0.098]	[0.080]	[0.118]	[0.093]
Observations	227	227	227	227
R-squared	0.274	0.269	0.233	0.260

Panel B: Dependent variable: Logged average daily wages (8 months)

Variable:	Ability	Any experience	International employer experience	Research experience
	(1)	(2)	(3)	(4)
Got a job	0.715**	0.616	0.325	0.076
	[0.351]	[0.664]	[0.354]	[0.420]
Got job X `Variable'	-0.155	0.177	1.187**	1.301*
	[0.317]	[0.786]	[0.541]	[0.706]
`Variable'	0.005	0.099	0.234	-0.464
	[0.290]	[0.383]	[0.332]	[0.389]
Observations	165	165	165	165
R-squared	0.230	0.223		0.205

Notes:

The probability of alternative employment (P_i) and the interaction of the baseline characteristic and the probability of alternative employment assigned ($Base_i * P_i$) are used to instrument for the binary indicator JO_i and the interaction of the baseline characteristic and the job offer ($Base_i * JO_i$). The fraction months employed variable is calculated as the number of months the individual was employed over the last 8 months, divided by 8. The average daily wage is calculated using the restrospective job work history. Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 10 percent level. Robust standard errors are reported.

Table 7: Shifts in occupations

Panel A: Any job held in occupation type during post-intervention period

	Professional, technical, and related workers	Administrative and managerial workers	Clerical and related workers	Sales workers	Service workers	Agriculture, animal husbandry, and forestry workers, fishermen, and hunters	Production and related workers, transport equipment operators, and labourers	Research Assistant position
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Got a job or recruiters job offer (IV)	0.157	0.050	0.147	-0.011	-0.051	-0.034	-0.030	0.154*
	[0.108]	[0.039]	[0.107]	[0.042]	[0.039]	[0.037]	[0.061]	[0.087]
Constant	-0.357	-0.133	-0.441	-0.074	-0.220	0.274	0.518	-0.510
	[0.425]	[0.094]	[0.372]	[0.182]	[0.163]	[0.290]	[0.367]	[0.313]
Observations	227	227	227	227	227	227	227	227
Ave of dep variable (no job)	0.392	0.013	0.300	0.062	0.053	0.057	0.084	0.205

Panel B: Number of month in occupation type in post-intervention period

	Professional, technical, and related workers	Administrative and managerial workers	Clerical and related workers	Sales workers	Service workers	Agriculture, animal husbandry, and forestry workers, fishermen, and hunters	Production and related workers, transport equipment operators, and labourers	Research Assistant position
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Got a job or recruiters job offer (IV)	0.665	0.272	0.671	0.125	-0.356	-0.156	-0.064	0.894**
	[0.596]	[0.245]	[0.476]	[0.186]	[0.258]	[0.228]	[0.293]	[0.422]
Constant	-2.211	-0.605	-0.514	-0.490	-1.202	2.943	0.993	-1.141
	[2.308]	[0.553]	[1.539]	[0.471]	[0.998]	[2.414]	[1.047]	[1.780]
Observations	227	227	227	227	227	227	227	227.000
Ave of dep variable (no job)	1.551	0.044	0.943	0.229	0.335	0.308	0.348	0.846

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 8: Mechanisms

Dependent Variable:	Unit of pay (1 = daily, 2 = weekly, 3 = fortnightly, 4 = monthly) (1)	Submitted any reference letter (2)	# times used any reference letter (3)	Any job referral (4)	Secured a job through referral (5)	Self-reported month reservation wage (6)	Minimum accepted wage (7)
Got a job or recruiters job offer (IV)	-0.684*	-0.086	-0.531	0.225**	0.082	123.911	3.640
	[0.385]	[0.110]	[0.608]	[0.112]	[0.055]	[87.831]	[4.137]
Constant	2.621**	0.072	-3.395	0.736	-0.099	1.236	8.135
	[1.164]	[0.480]	[2.920]	[0.456]	[0.170]	[201.305]	[13.303]
Stratification cell FE's	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166	227	227	215	214	221	165

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

The average daily wage is calculated using the restrospective job work history. The average daily wage is calculated as the average wage on the individual's main job in the last month. Columns 1 through 3, those who are unemployed are coded as 0's. Columns 4 through 6 uses the logged wage, therefore for individuals who earned $0 across all eight months are omitted. Ave hours worked per week is also calculated using the retrospective job history data. It is calculated as the average number of hours worked per week on the individuals' main job by month.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported

Appendix Table A: Sample and Attrition

	Baseline N=268			Covariate * Probability of
	Mean	SD	Covariate	Job offer
	(1)	(2)	(3)	(4)
Demographics:				
Age	25.604	4.638	0.004	0.001
Married	0.172	0.378	-0.031	0.136
Any child?	0.164	0.371	0.000	0.087
Number of children	0.299	0.784	0.028	-0.029
Years of education	13.183	0.940	0.064**	-0.104
Income (USD, 3 months)	206.123	228.803	0.00004	0.00001
Ability score	-0.001	1.003	0.035	-0.035
Tribe:				
Chewa	0.310	0.463	-0.064	0.093
Lomwe	0.108	0.311	0.125*	-0.304
Ngoni	0.164	0.371	0.057	0.138
Tumbuka	0.190	0.393	-0.041	0.112
Other	0.201	0.402	0.029	-0.188
Education and Work:				
Ever worked?	0.869	0.338	-0.014	-0.152
Ever worked with recruiter?	0.104	0.306	-0.093	0.107
Any work in last month	0.646	0.479	0.039	0.131
Any work in last 6 months	0.869	0.338	0.109	0.167
Frac of 6 mths worked	2.657	2.176	0.008	0.015
Any job search last month	0.116	0.320	-0.085	0.270**

Notes:

The baseline sample consists of 268 individuals who participated in the recruitment process and experiment discussed in Section 2. Columns 3 and 4 are from the same regression predicting where the dependent variable is whether or not the individual was found at follow up. Columns 3 and 4 present the coeffieint on the baseline characteristic and the interaction of the baseline coefficient and the assigned probability of a job offer respectively .